Anonymous Referee's comments #1

We thank Anonymous Referee #1 for providing useful and constructive comments. We have carefully revised the manuscript and addressed all the points raised by the Referee.

General Comments:

This study investigates the vegetation response to drought by looking at the correlation between LAI and SPEI and compares the responses between the observed and modeled world. Overall, this study is very interesting, and the manuscript is well organized and reads relatively clearly.

Response:

Thank you for the feedback.

I do think the authors could explore deeper in discussing the differences of the observed and simulated vegetation response to drought and highlight the possible implications on model development in terms of better capturing the vegetation response to drought. Please see my specific comments below:

Response:

Thank you. We added a discussion of the possible reasons why models tend to overestimate vegetation response to drought and made suggestions on the future scope of model developments in a newly added Section 4.6, Lines 825 – 849, Page 30. This is copied below under specific comments number 10.

Specific comments:

1. Line 154-156: Is there a specific reason of choosing the period of 1982-2011 for this study? Why not extending to 2019?

Response:

The present study extends the timeframe for understanding drought impacts from 1982 to 2011 mainly because there were frequent droughts in the 2005 – 2011 window (Masih *et al*., 2014). The timeframe was then extended back to cover a 30 year period to be long enough to cover impacts of climate change, which is particularly important considering that southern Africa experiences more frequent droughts with impacts exacerbated by climate change. This information is important for considering adaptation measures and understanding the role of climate change. Please see Lines 241 – 247, Page 7.

2. Line 84: please check the reference, it seems that the paper is published in 2010 instead of 2005. Besides, is it possible to update the reference to recent advance reflecting the statement of "southern Africa may lose about one-third of its current vegetation due to increasing exacerbation of drought in the region"?

Response:

Apologies for this, and thank you for the correction. The date has been updated in the revised manuscript.

The sentence "southern Africa may lose about one-third of its current vegetation due to increasing exacerbation of drought in the region" has been updated to a recent reference. The statement now reads *"It is reported that there has been significant loss of vegetation cover over the region over the last 30 years (Driver et al., 2012; DEA, 2015)"*. Please see Lines 81 – 82, Page 3.

3. Line 264-265: Do you also deseasonalize the simulated LAI before the correlation analysis?

Response:

Yes, we also deseasonalized the simulated LAI before correlation analysis. This was done to make appropriate comparisons. This has been clarified in the manuscript. Please see Lines 305 - 306, Page 8.

4. Line 273-275: Is there a major difference between the CRU and CRUJRA datasets in terms of the precipitation and temperature fields? If so, what are the differences?

Response:

The major difference between CRU and CRUJRA is in terms of the spatial and temporal resolutions. CRU is gridded 'observed' data, although it is limited by the fact that temporal resolution is monthly. JRA is a reanalysis and has 6-hourly temporal resolution. JRA is reanalysis but the combined product uses the sub-monthly information from JRA and is constrained to the monthly CRU observations. With regards to the precipitation and temperature fields, the difference is negligible for southern Africa. Please see Figs. 5 for the spatial comparisons of the data. This text can be found under Section 4.2, Page 26 – 27, Lines 681 – 693.

For the study, we used CRUJRA because it is the data used to force the DGVMs, so the drought indices are being calculated based on the same data the models use for their simulations. It is useful to use data with shorter times because the study focuses on an evaluation of drought impact, which is sensitive to timescale. In drylands, for instance, the uncertainties associated with monthly data in drought monitoring are reduced when sub-monthly data are used (Mukherjee *et al*., 2017). Also see Section 4.2, Page 26 – 27, Lines 681 – 693.

5. Line 277-282: The description is a bit confusing. Do you calculate the correlation for each month separately using the 30 years data and then calculate the seasonal mean of the correlation? Please refine the description.

Response:

Apologies for the confusion.

Yes, we calculated correlation (twelve sequences in a year) of monthly LAI to monthly sequences of 1- to 24-months SPEI using the 30-years data. Seasonal mean of the correlation was then calculated. This has been added in the revised manuscript. Please see Lines 321 – 328, Page 9.

6. Section 3.1: How about the correlation between the simulated and observed monthly LAI time series? Figure 2 has indicated that the spatial pattern between the simulated and observed LAI matches relatively well, but I wonder how they compare to each other in terms

of seasonal and interannual variation? I guess this might be helpful when explaining the difference between the vegetation response to drought in the observed and modeled space?

Response:

Thank you for the suggestions. Figure 2 illustrates a spatiotemporal correlation, incorporating both the spatial and temporal patterns between observed and simulated LAI. We also have made comparison of the annual cycle of LAI for observation and models (TRENDY) across six biomes over southern Africa for the period 1982 – 2011 as shown in Fig. 3 (now Fig. 4) - Please see Lines 431 – 435, Page 14, and Fig. S5.

Based on your suggestion, we have now added a **new Fig. 3 (shown below)** to separate spatial and temporal patterns and highlight interannual variation. Figure 3 shows spatial seasonal distribution and inter-annual variability (IAV) of satellite-calculated and modelled LAI (multi-model mean) over southern Africa. (A) – (D) show the difference (bias); (E) – (H) and (I) – (L) show their standard deviation (Stdev); (M) – (P) show their spatiotemporal correlations; and (Q) shows their inter-annual variability for the period 1982 – 2011 (as copied below). Please see Lines 385 – 391; Page 12 of the revised manuscript for this information. The analyses were adopted after Luo *et al* (2013). A discussion on this has been added to the text.

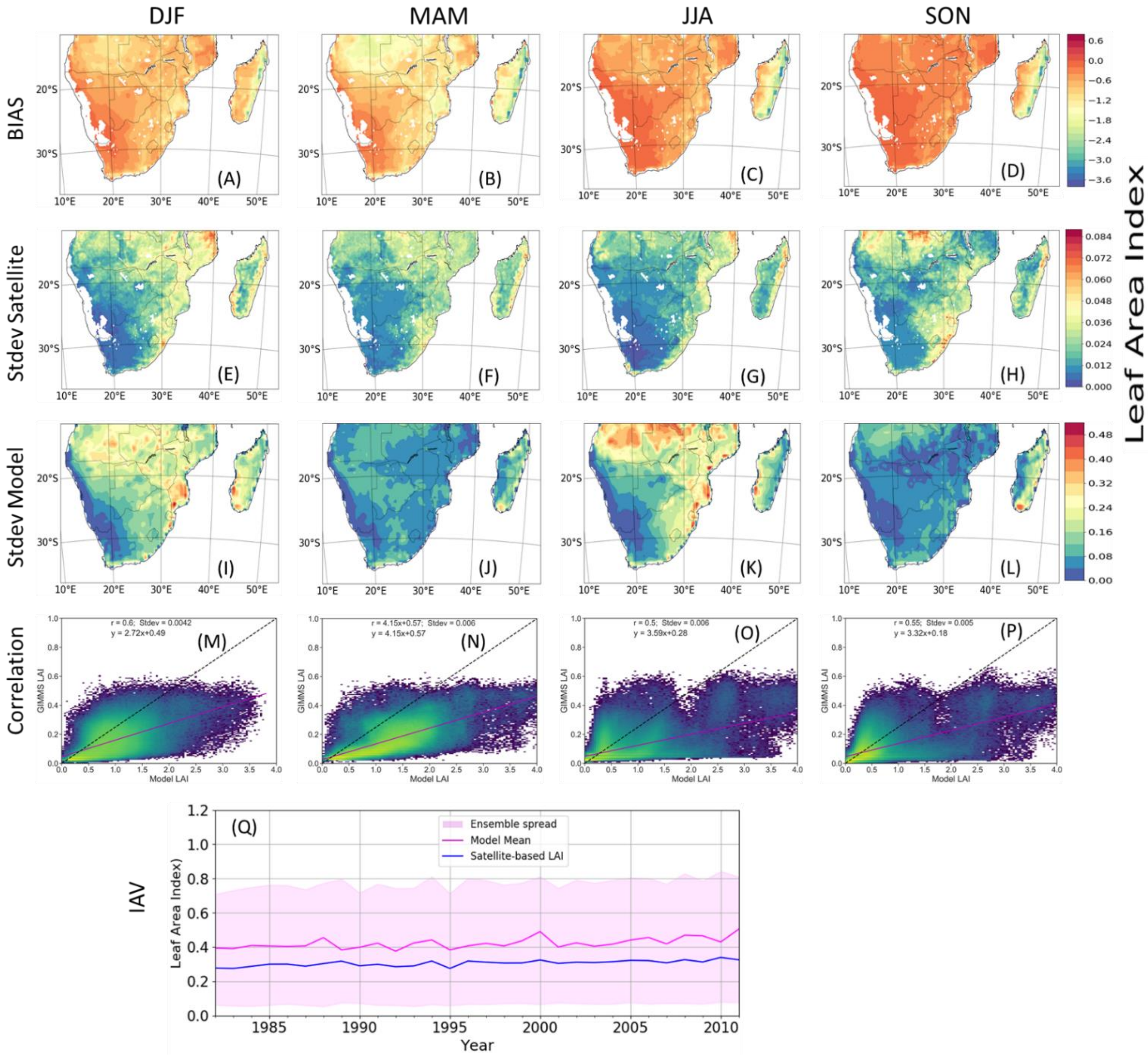

Figure 3. Spatial seasonal distribution and inter-annual variability (IAV) of satellite-calculated and modelled LAI (multi-model mean) over southern Africa. (A) – (D) show the difference (bias); (E) – (H) and (I) – (L) show their standard deviation (Stdev); (M) – (P) show their spatiotemporal correlations; and (Q) shows their inter-annual variability for the period 1982 – 2011.

7. Figure S5: It is interesting that for Mediterranean and Tropical forest, models almost fail to simulate the second peak around September. Any possible explanations?

Response:

Thank you for noting this. For Fig. S5, some of the models do not capture this peak around September over the Mediterranean and Tropical biomes. A possible explanation is that the models do not well reproduce the changes in the biomass and leaf area cover around that period (due to phenological responses to environmental variables). For instance, some models may simulate leaf-off for stress deciduous vegetation types prior to September.

For both biomes, spring rainfall contribute to vegetation growth in the region, which may not be well reproduced by the models. These have added to the text. Please see Section 4.3, Lines 709 – 719, Page 27.

8. Line 328: While the first sentence mentioned that "This section compares the seasonal cycle of observation (CRU) and reanalysis (CRUJRA) climate variables. . .", I only see one set of climate variables in Figure 3. Are they from CRU or CRUJRA? And there is no discussion with respect to the comparison between the two. Please consider add on the corresponding figures/analyses.

Response:

Apologies and thank you for pointing our attention to it. The set of climate variables now include CRU and CRUJRA. We have made the necessary correction in line 395, Page 12.

We made spatial comparison of both CRU and CRUJRA in Fig. S6 (and Fig. 5). As you have suggested, we have added corresponding figures/analyses. This can found in in Lines 436 – 440, Page 15 of the revised manuscript. The discussion has been modified to reflect the changes. Please see the adjustment of the figure (now Fig.4) below:

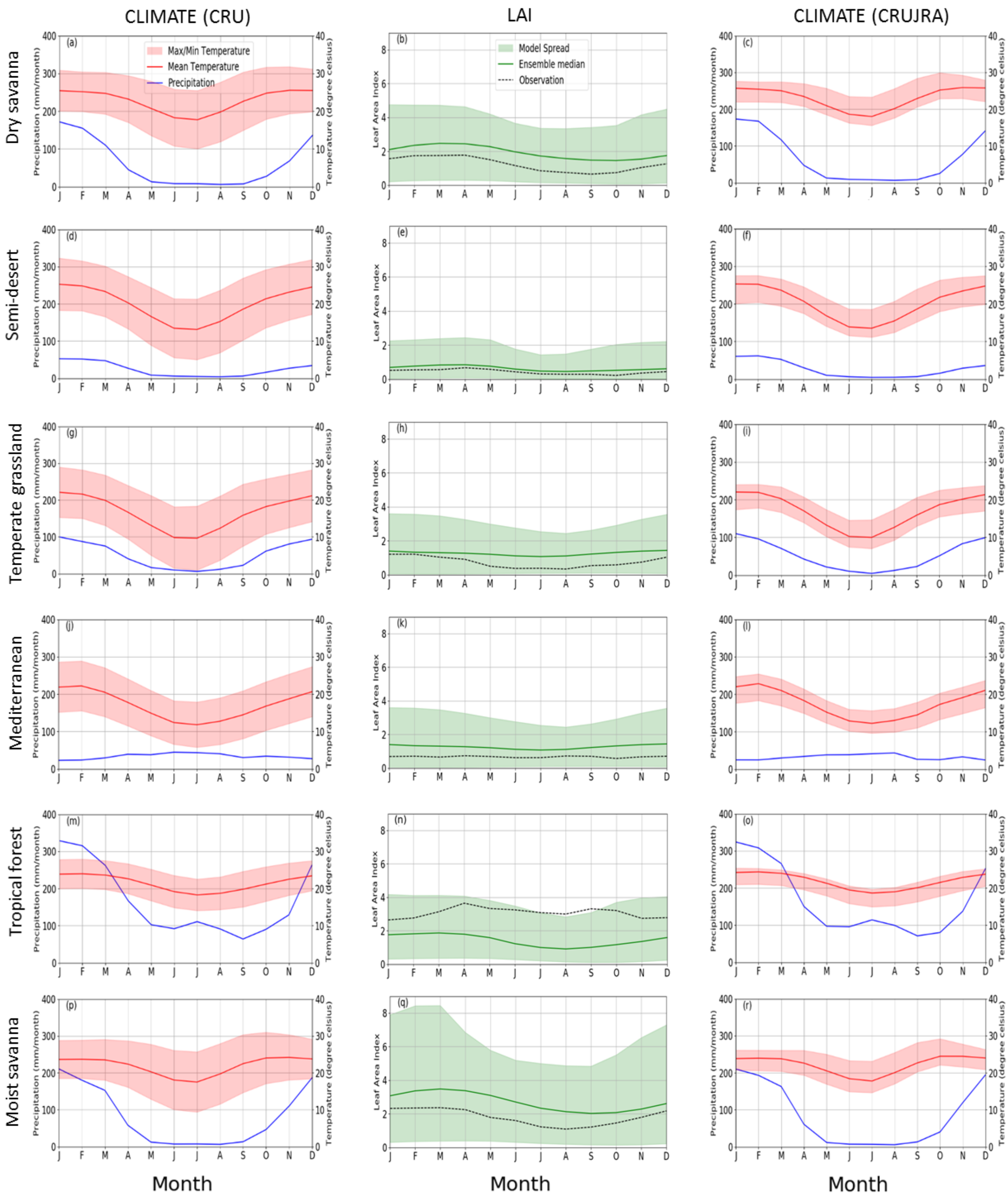

Figure 4. Annual cycle of observed climate variables (precipitation, mm/month; maximum, minimum and mean temperature, °C) and LAI for observation and multi-model mean (TRENDY) across six southern African biomes over for the period 1982 – 2011. The annual cycle of the LAI for individual models are shown in Figure S5.

Furthermore, we should note, that a comparison of the spatial distribution of CRU and CRUJRA (formerly Fig. S6) has also been moved to revised manuscript, based on the suggestion of reviewer. This figure is now Fig. 5 and is shown in Lines 436 – 440, Page 15.

9. Line 375: "The severity of drought intensity is similar for all SPEI" – which character in Figure 4 do the authors refer to?

Response:

We were referring to the magnitude of the severity which is on the y-axis of 1-month to 24-month SPEI. This is now clarified in the text. Please see Lines 454 - 455 Page 16.

10. Section 3.4 & 4.4: This finding is very interesting. I think it's worth to populate the discussion regarding possible reasons why models tend to overestimate the magnitude and time scale of vegetation response to drought and advice on future scopes of model developments.

Response:

Thank you. We added a discussion of the possible reasons why models tend to overestimate vegetation response to drought, and made suggestions on future scope of model developments in a newly added Section 4.6 Please Lines 825 – 849, Page 30. The text is copied below:

4.6 Variations in observed and simulated vegetation response to drought, and implications on model development

The biases shown by models could be attributed to the different limitations of individual DGVMs, and addressing these shortcomings would improve models' performances. For example, the sub-optimal performance of CLM may be partly due to the inability of the model to capture foliage production and root system of vegetation for transpiration. The model is also unable to produce savanna ecosystems, which it simulates by approximating vegetation of forest and grassland ecoregions (Dahlin et al., 2020). In addition, the ineffective simulation of deciduousness would have contributed to the model biases in response simulations. Therefore, targeting these limitations is important for improving model's performance in simulating morphology and physiological functioning of vegetation biomes. Furthermore, the DGVMs (e.g. JULES, DLEM) used in the study poorly replicate important ecological and physiological processes that are critical to capture the dynamics of savanna systems. Other DGVMs (e.g. JSBACH) poorly simulate significant environmental variables such as fire, which is very crucial for the vegetation growth cycle, particularly in the savanna biome (Thonicke et al., 2001; Romps et al., 2014; Kim et al., 2018; D'Onofrio et al., 2020). Also, over southern Africa, land use change (LUC) is a common and frequent occurrence, and is an important factor for vegetation turnover. However, most models do not well capture land management, which is an important driver of land cover change in the region. Thus, there is a need for future model development to account for rapid LUC over different regions. However, the disparity in observed and simulated response of vegetation to drought cannot be fully accounted for by the DGVMs alone. The reanalysis (CRUJRA) has also shown some limitations in the simulation of climate variables. Compared to observation-based CRU, CRUJRA has closer magnitudes of maximum and minimum temperature, and addressing this would improve simulated response.
Please see lines 829 – 850, Page 30.

11. Section 3.5: Why stratify the analyses based on latitude instead of the vegetation biome types?

Response:

We stratified the analyses based on latitude and investigate and identify the shift in response based on the vegetation types across the latitudinal belt. We have made clarifications on this in the revised manuscript. Please see Lines 505 – 508, Page 19.

We also classified our analyses based on vegetation types. Please see Figure 9 (formerly fig. 7).

In summary, we did both.

12. Figure 6: is the red line represent "ensemble median" or "ensemble mean"? I noticed that sometimes "ensemble mean" is used and sometimes "ensemble median" is used. Please check and clarify across the text/figures.

Response:

For Fig. 6 (now Fig. 8), the red line is ensemble mean. Ensemble median was used in Fig. 5 (now Fig. 7), where we calculated correlations. We have made further clarification across the text and figures. Also see Lines 509 – 524, Page 19 – 20.

13. Figure 7: While models overestimate the drought time scale for most of the vegetation biomes, it seems that they tend to underestimate the time scale for Dry savanna. Any ideas on what might be causing this difference?

Response:

A possible reason for the difference why the time scale for Dry savanna was underestimated may be because phenological triggers for dry savannah vegetation types respond differently to environmental variables, which the models do not capture. The African Dry savanna region is characterized by rapid vegetation changes due to fire, land-use among others, as well as senescence for prolonged dry periods (Rahimzadeh-Bajgiran *et al*., 2012; Zhu and Liu, 2015). Similarly, models all represent fire differently, and this could contribute to model responses. This has been added to the text. Please Lines 542 – 548, Page 21.

14. Table 1: It seems that CLM performs the worst among others. Could the authors explore a bit on why this is the case?

Response:

We plan to explore the reasons for this in future investigations. A possible reason for the weak performance of CLM may be its representation of the canopy construction of the PFTs and of its foliage clumping representation. In addition, CLM is limited in its simulations of vegetation with regards to transpiration, due to rooting among others (Dahlin *et al*., 2020). Furthermore, CLM does not well simulate savanna ecosystems, but instead uses a combination of grasses, shrubs, and trees. There are also some problems (such as an unusual green-up in dry season) identified with stress deciduous responses (Dahlin *et al*., 2015). Please see Lines 589-594, Page 23.

15. Section 3.8: Could the authors elaborate on how the impact of extreme events are evaluated and the rationale behind? For instance, how the correlation of a wet year of 2000 is calculated? I have trouble understand how the response for a single year is projected onto the response for a longer time span.

Response:

Apologies for the confusion. The objective was to investigate the impacts of extreme hot and dry years on LAI. In order to understand this, we selected very dry years and compared the response during these periods to wet years. We investigated the response in the individual years without projecting onto the response for a longer time span. Our method was adopted after Pan *et al* (2015). Clarifications have been made in the revised manuscript. Please Lines 599 – 613, Page 24.

Technical corrections:

16. Line 220: A period is missing after ". . .understanding drought impacts through 2011".

Response:

Thank you for the correction.

17. Line 244: Please correct for the typo – "Penman-Monteith".

Response:

Thank you.

18. Line 275: A period is missing in the end.

Response:

Noted. Thank you.

19. Line 419: Typo: "magnitude"

Response:

Thank you.

20. Line 589-590: Should be "Fig. S6"?

Response:

Thank you for the correction.

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Anonymous Referee's comments #2

We thank Anonymous Referee #2 for providing useful and constructive comments. We have carefully revised the manuscript and addressed all the points raised by the Referee.

The paper, "Investigating the response of LAI to droughts in southern African vegetation using observations and model-simulations" describes a study in which the authors compare the standardized precipitation evapotranspiration index (SPEI) to Leaf Area Index (LAI) based on (1) satellite observations and (2) dynamic vegetation models run using reanalysis data. The authors compare the performance of these indices calculated for different time lengths and averaged over different biomes in southern Africa.

Response:

We offer a clarification: The goal of the study was to examine how the LAI responds to drought as represented by the SPEI in observation-based data and model simulations.

A comparison was made between climate variables (i.e. precipitation and temperatures) and the LAI to show how the climate variability of the different variables is driving vegetation (LAI) growth over southern African biomes. Another comparison was made between satellite-based (hereafter, observed LAI / observation-based LAI) and simulated responses of the LAI to drought.

We have made efforts to update the text to clarify that we are estimating drought using SPEI.

General comments: 1. Although the paper is titled "investigating the response of LAI to droughts" and the authors mention several times in the paper various analyses they undertake with respect to drought, the authors do not actually show how LAI responds to drought in the paper.

Response:

An investigation of LAI response to drought at different timescales was undertaken in the manuscript. We correlated LAI to SPEI, using an analytical method that was in line with previous studies (Hao *et al*., 2013; Vicente-Serrano *et al*., 2013; Hadian *et al*., 2013; Zhang *et al*., 2016; Khosravi *et al*., 2017; Zhao *et al*., 2018). These studies calculated the correlations between a vegetation index and the SPEI to investigate the response of a vegetation index to drought. Please see Section 3.4, Pages 18 - 19, Lines 475 – 502; Section 3.5, Lines 503 – 524; Pg 19 – 20; Section 3.6, Lines 525 – 578; Pg 20 – 22; Section 3.7, Lines 579 – 595, Pg 23; Section 3.8, Lines 599 – 613, Pg 24; & Section 3.9, Lines 615 – 644, Pg 24 - 25.

These sections detail analyses and results of LAI response to drought.

Please see a quick recap of our analyses below:

1. Grid cell correlations between NDVI and LAI: We computed grid cell spatiotemporal correlation between GIMMS LAI and GIMMS NDVI to evaluate the relationship between both indices. Although there is a strong linear relationship between the NDVI and LAI in Southern Africa, other studies (Potithep *et al*., 2010; Towers *et al*., 2019) have shown the two indices are not always directly proportional. For example, both indices do not exhibit the same relationships over different eco-regions such as the Evergreen Broadleaf Forest, Deciduous Needleleaf Forest. Furthermore, another study (Fan *et al*., 2009; Tian *et al*., 2016) found that the LAI may be better indicator of plant

biomass and health because of the saturation associated with the NDVI, particularly in drylands.

2. Climatology of observed and, simulated climate variables and LAI – We performed this task to examine how well the LAI is simulated by the models i.e. Dynamic Global Vegetation Models (DGVMs). This is where we compared climate variables (i.e. precipitation and temperatures) and the LAI. The climate variables are used to compute drought index but are not SPEI.

3. The evolution of drought in southern Africa: We plotted a time series of the evolution of drought for the 30-year period. The objective was to examine the inter-annual variation drought at different timescales (i.e. 1- to 24-month).

4. Spatial distribution of LAI response to drought and the timescales: Here, we look at the response of LAI to drought (SPEI), at different timescale, using correlation analysis. The analyses was adopted after previous studies such as Vicente-Serrano *et al*., 2013; Vicente-Serrano *et al*., 2013; Hadian *et al*., 2013; Zhang *et al*., 2016; Khosravi *et al*., 2017; Zhao *et al*., 2018; Hao *et al*., 2013, among numerous others.

5. Latitudinal distributions of LAI response to drought and the timescales - We analysed LAI response to drought based on latitude, first, because we intend to investigate and identify the shift in response based on the vegetation types across the latitudes.

6. Response of LAI to droughts across seasons – This was to see how LAI respond to drought at different seasons. Vegetation in southern Africa are seasonally variable. Numerous studies (Chamaille-Jammes *et al*., 2006; Rowhani *et al*., 2011; Thornton *et al*., 2011; Poulter *et al*., 2014) have shown that the seasonality of southern vegetation production follows the regional seasonal patterns of precipitation. Variability in temperature also affects the seasonal productivity of vegetation in these southern African biomes.

7. Inter-annual variation of model simulation of drought impacts on LAI – We investigated the variation in the inter-annual simulation of LAI response to drought across different timescales by individual models.

8. Impacts of extreme events on LAI: The objective was to discuss the impacts of extreme event during extreme hot and dry years, on LAI.

9. Comparison of global and regional distribution of LAI response to droughts (1982 – 2011): We investigate the variability in the global and regional temporal distribution of LAI response to drought.

They use a drought index, SPEI, but these indices typically need classification schemas to define when there is a drought or not, and how severe the drought is (similar to how WMO classifies different drought severities based on SPI thresholds). So one would expect some thresholds to be defined in the SPEI as drought thresholds. This was never done in the paper, but rather the paper is a comparison of SPEI and LAI, and never states which periods does LAI correctly identify the presence (or absence of drought), or its severity, which is the type of analysis one would expect in a drought analysis.

Response:

Thank you for pointing out that the text is unclear in this regard. Most of the analyses in the manuscript focused on the response of LAI to drought, and we have updated the text to clarify. The thresholds of SPEI are defined as shown in Fig. 6 in Section 3.3, Lines 445 – 465, Pages 16 – 17. The figure shows the drought thresholds of SPEI, at different timescales over southern Africa (This is similar to studies such as Vicente-Serrano *et al*., 2013; Zhao *et al*., 2018; Hao *et al*., 2020. In addition, we have now added a table (now Table 1) on the definition of SPEI thresholds to the revised manuscript (also copied below) in Lines 286 – 289 We should also note that the SPEI (unlike SPI or PDSI) is the most appropriate index for measuring drought in southern Africa, as it accounts for the effect of evaporative demand from the atmosphere in drought monitoring (Vicente-Serrano *et al*., 2010; Ujeneza, 2014). In addition, the SPEI is reported to be able to identify the geographical and temporal coverage of droughts (Vicente-Serrano *et al*., 2010; Ujeneza *et al*., 2014).

Furthermore, our analyses show which periods LAI responds to the presence/absence as well as the severity of drought. This is referred to as “drought time scale” in the manuscript (see Figs. 9 - 11). The text has been added to the revised manuscript in Lines 301 – 302, Page 8. The comparison was made between the satellite-based and simulated response of LAI to drought, and not between the SPEI and the LAI. LAI is a vegetation index while SPEI is a drought index, and our analyses did not compare the different indices.

Table 1. Definition of drought thresholds based on the SPEI scale

SPEI	**Drought thresholds**
2 or more	Extreme wet
1.5 to 1.99	Severe wet
1 to 1.49	Moderate wet
0 to 0.99	Mild wet
0 to -0.99	Mild drought
-1 to -1.49	Moderate drought
-1.5 to -1.99	Severe drought
-2 or less	Extreme drought

Source: Wang *et al*. (2014), modified as shown in Lawal, 2018.

2. The authors are encouraged to proof-read the paper to reduce several typographic/grammatical errors found in the paper including apparently incomplete sentences.

Alternatively, use of a professional editing service could be taken. I have included a few examples in the specific comments below, but the authors need to please do a thorough proofing of the entire document before submission.

Response:

Apologies for the typographical/grammatical errors. We have revised the manuscript carefully to fix grammatical errors.

3. A basic expectation of papers being submitted is that all citations used in the paper should be included in the list of references, and should be cited and referenced correctly. This is not the case, several citations have not been included in the references (e.g. citations on line 82, 90, 91 are missing from the references, and on line 101 the date in the citation does not match the date in the references. This is all just on the 1st page of the manuscript, the authors should check all the citations and references in the manuscript before submission.

Response:

Apologies for missing some citations in the references. We have checked and updated this in the revised manuscript.

Specific comments: Line 81. "It is reported that there are now fewer vegetation coverage", should probably read "It is reported that there is now less/reduced vegetation coverage"

Response:

Thank you. We have reworded the phrase as you have suggested. It now reads:

"*It is reported that there has been a significant loss of vegetation cover over the region over the last 30 years (Driver et al., 2012; DEA, 2015)*". Please see Lines 81 – 82, Page 3.

Line 81-82. "It is reported that there are now fewer vegetation coverage in the region compared to what existed between the mid 1990's and 2000's (EOS, 2007)." I am failing to understand how this reference, published 2007, can compare what is happening now (in 2020) with what happened in the 2000s.

Response:

We have now updated this sentence according to a more recent references. Please see Lines 81-82, Page 3.

Line 120-125: I understand the reasons given for using LAI over NDVI. But given that satellite LAI in this study seems to be derived from NDVI, does this then not invalidate the point made by the authors. Also, given that the authors need to indicate how well does the satellite derived LAI they use estimate actual, ground-measured LAI, or at least with independent, acknowledged accurate LAI estimates.

Response:

The satellite-based LAI and the NDVI, used in this study, are two different indices. The LAI was post-processed using different data (MODIS LAI, fPAR, AVHRR NDVI) for the period of 2000-2009. The GIMMS LAI used in the study covers 1982-2011. Furthermore, GIMMS LAI product is superior here over the GIMMS NDVI, which is due to the information derived from the MODIS LAI. The additional properties on GIMMS LAI by MODIS differentiate the index from NDVI. Thus, it was necessary to investigate how the two indices differ. Please see Lines 182 – 190, Page 5.

In addition, studies *(Potithep et al., 2010; Towers et al., 2019) have shown that LAI and NDVI are not always directly proportional. For example, these indices do not exhibit the same relationships over different eco-regions such as the Evergreen Broadleaf Forest and the Deciduous Needleleaf Forest. Furthermore, another study (Fan et al., 2009; Tian et al., 2016) found that the LAI may be a better indicator of plant biomass and health because of the saturation associated with the NDVI, particularly in drylands.* This makes the LAI more applicable in monitoring vegetation response to drought. Thus, our manuscript makes a substantial contribution in evaluating how the LAI differs from the NDVI over different biomes (such as dry savanna, tropical forest, etc). See Section 4.1, Lines 665 – 671, Page 26 for this text. Also, please see Figs. 2, 3, 4, S5 and S7 for more information.

With respect to how well satellite-derived LAI (hereafter observation-based /observed LAI) estimate actual, ground-measured LAI, this has been extensively investigated at different geographical locations and reported in numerous studies (please see Rezaei *et al*., 2016., Forkel *et al*., 2013., Schaefer *et al*., 2012., Lawal *et al*., 2019a). Please see Lines 189- 191, Page 5 for the text.

Yes, we have investigated how well the satellite LAI estimate measured LAI over southern Africa. For the region, the only ground-measured LAI available is over the Kwazulu Natal province (28.533°S, 30.9°E) and can be obtained from the global database of field observed LAI in woody plant species: https://daac.ornl.gov/VEGETATION/guides/LAI_Woody_Plants.html). The data covers the period from 2000 – 2004. The data is only representative of a very small region and covers only one biome. Moreover, it covers only a short temporal period. Therefore, validating the satellite LAI data with the ground-based data at larger scale is not possible. Note, this has been reported in a previously published study such as Lawal *et al*., 2019a. The results are not shown here to avoid duplication.

Due to the limitations of in-situ measured LAI (missing data, low spatial coverage, short temporal period, among others), satellite LAI has been widely used in the monitoring of drought (please see Thenkabail *et al*., 2004; Aghakouchak *et al*., 2015; Kim *et al*., 2017).

Line 152: "These data were gotten from CRU..." would read better as "These data were obtained from CRU..."

Response:

Thank you. Revised.

Line 153-162: Regarding the use of CRU and CRUJRA data, the authors need to state either through their own analysis or quoting other studies how well does CRU and CRUJRA compare with actual meteorological data over southern Africa since they are using these CRU/JRA data to calculate "ground-truth" for drought (although noting that drought was not calculated by SPEI).

Response:

We have added the following statement to the manuscript:

"*Previous studies (e.g. New 1999; New 2000; Wolski et al., 2018; Otto et al., 2018; Harris et al., 2020) have shown that there is a good and robust agreement between observation network and CRU over most parts of southern Africa. We should note that sparseness and missing data generally affect the correlation between CRU and station data in the region. Furthermore, with respect to inter-annual variability, CRU robustly captures the climate factors in southern Africa. The major exception is with the long-term trend of precipitation particularly over Western Cape province of South Africa as well as wetter than normal condition over the same province. These limitations do not affect the validity of our results because we are looking at below-normal precipitation and temperature*". Please see Lines 155 – 163, Page 5.

Since CRUJRA is derived from CRU (as we have discussed earlier), the agreement generally persists. The spatial and temporal coverage of the study is the reason for using CRU and CRUJRA but not station data.

Conducting another set of analyses to evaluate CRU and CRUJRA falls outside the scope of this study, because this already has been done in numerous other studies. Furthermore, it has been one of the most widely used datasets for more than two decades.

The computation of SPEI using CRU gives a robust representation of drought as we have discussed above and has been shown in other numerous studies (Vicente-Serrano *et al*., 2013; Hadian *et al*., 2013; Zhang *et al*., 2016; Khosravi *et al*., 2017; Zhao *et al*., 2018; Hao *et al*., 202).

Line 173: "The trained neural networks were then used to produce the LAI3g and FPAR3g data sets". This description suggests that the LAI and FPAR data were derived from NDVI data. If so, then this needs to be expressed explicitly. MODIS LAI, which was used for training, is also in part derived from NDVI. This all implies that as the LAI is a derived dataset, how accurate is it in depicting actual LAI.

Response:

The sentence has been revised to clarify how LAI is partly post-processed from the NDVI for the period of 2000-2009. . Please see Lines 182 – 190, Page 5.

As stated in our earlier response, satellite-calculated LAI (hereafter, observed LAI / observation-based LAI) have been evaluated with measured LAI values where the latter is available. The lack of available data at a significant spatial and temporal scale makes it difficult to comprehensively evaluate GIMMS LAI with measured LAI. Please note that a measured LAI value is only available from the Kwazulu Natal province (28.533°S, 30.9°E). However, validation of satellite data and modelled vegetation data have been discussed for different regions in Parasuraman *et al*., 2007., Rezaei *et al*., 2016., Forkel *et al*., 2013., Schaefer *et al*., 2012., Lawal *et al*., 2018, 2019a.

Line 192-193: "This was necessary to show ... how well the models simulate the LAI in the region." The authors need to provide evidence of how accurately the GIMMS LAI estimates actual LAI in the region before they can make this determination.

Response:

Previous studies (Fan *et al*., 2009; Tian *et al*., 2016) have shown that the LAI is a good estimator of vegetation health. *The lack of available data makes it difficult to compare GIMMS LAI and actual LAI. Nevertheless, the GIMMS LAI has been evaluated and agrees well with observations in other regions* (*Fan et al., 2019*). Please see Lines 210 – 212, Page 6.

Line 201-204: The authors stated that they adopted a definition of drought that is defined based solely on precip, but then throughout the paper, they used an index based on PET as well as a proxy for drought. This discrepancy needs to be reconciled in the text (ie, please redefine drought on lines 201-204 to match how it is used in this paper). Additionally, the authors need to state the threshold they use for defining drought, given that this is an index-based drought analysis.

Response:

Thank you. Although the definition of meteorological drought adopted in the study is based on the magnitude of precipitation below long-term normal, it encompasses other meteorological factors such as temperature, humidity, wind, among others. The deficit in magnitude of precipitation compared to long-term normal is accounted for by PET (i.e. potential evapotranspiration). This is the reason we used SPEI, which is an index based on PET. This clarification has been added to the manuscript. Accounting for the influence of PET on drought in a region such as southern Africa gives a robust drought monitoring, as shown in other studies and stated earlier. Generally, the most accepted measure of drought is to compare precipitation magnitudes, while also accounting for factors that may have influenced changes in magnitudes. An appropriate index that holistically captures this definition of drought or drought event is the SPEI.

As stated earlier, drought threshold based on SPEI varies from +2 to -2, with the former indicating an extreme wet condition and the latter indicating an extreme dry condition (see our new table, Table 1 in Line 286 – 289). The "drought timescale" referred to in our study accounts for the period in which LAI is affected by drought.

We note that the methodology used in this paper follows what has been used in numerous studies e.g. Vicente-Serrano *et al*., 2013; Hadian *et al*., 2013; Zhang *et al*., 2016; Khosravi *et al*., 2017; Zhao *et al*., 2018; Hao *et al*., 2020, among others.

Line 229: While P and PET are defined in the text referencing the equation (line 226- 227), D is not defined likewise, please define.

Response:

Thank you. We have clarified.

The phrase has been added to manuscript: *"D-values represent a measurement of water deficit or surplus aggregated at different time scales"*. Please see Lines 254 – 256, Page 7.

Line 231-233: This sentence should be moved to line 243 which is already providing more details about how PET was calculated. Additionally, the section should state which data was used for max, min and mean temperature.

Response:

We have moved the statement in lines 231-233 to lines 263 – 265, Page 7.

The data used for max, min and mean temperature are CRU and CRUJRA based (as stated earlier in lines 149 - 151). This has been clarified throughout the manuscript as suggested.

Line 237-240: In these lines, the authors provide a quotation without clearly attributing it to anyone. Can the authors please add additional text to attribute the quotation. Also, for the sake of completeness in defining SPEI, this information would be better summarized in equation form and presented together immediately after equation 1 in the form "SPEI = ... (eq 2)".

Response:

Additional text has been added to attribute the quotation. Please see below.

"For the 1-month timescale, only the current month data is used for the calculation. The D values were standardized by assuming a suitable statistical distribution (e.g. gamma, log-logistic). The log-logistic distribution was used to standardize the D values in this study" (Lawal et al., 2019a). Please see Lines 259 – 262.

Equation 1 in the text has summarized the definition of SPEI.

Line 248: "The study...". Which study is being referred to here, is it the manuscript's study, or Lawal et al (2019) - this needs to be clarified.

Response:

It is the previous study by Lawal *et al* (2019a), which we now clarify in the revised manuscript.

Line 252: "such" should be "such as"?

Response:

Done.

Line 259-260: given per-pixel correlation, how did you deal with the difference in spatial resolution of the CRU (0.5 deg) and GIMMS (8km)

Response:

We regridded the data to the same spatial resolution using the bilinear interpolation method. This has been clarified in the manuscript. Please Lines 296 – 297, Page 8.

Line 259: Reference is made to a "drought index (CRU)". which one is the CRU drought index - it has not been referred to before. Is this referring to SPEI? If so, it may be better to just say SPEI (based on CRU data), to avoid confusion of the reader.

Response:

Thank you. We have changed "drought index (CRU)" to SPEI (based on CRU). Please see Line 296, Page 8.

Line 268: Citation not in the references.

Response:

Apologies. This has been added to references.

Line 277-282: This explanation is quite unclear to me. It sounds like they average out correlations. Can the authors please rephrase to make this clearer.

Response:

Apologies for the confusion.

The following statement was added to the manuscript.

"*In simpler terms, we calculated the correlations (twelve sequences in a year) of monthly LAI to monthly sequences of 1- to 24-months SPEI using 30-years of data. Subsequently the seasonal mean of these correlations was calculated*". Please see Lines 323 – 326, Page 9.

Line 294 (Figure 1). "Contours" should be "lines".

Response:

Thank you. "Contours" have been reworded as "lines".

Line 305-306: "The low standard deviation indicates that the values from the two indices are close and a standard lower deviation." I don't understand this sentence. Please rephrase and make it clearer. Additionally, the standard deviation being re-ferred to is for which parameter - this needs to be clarified.

Response:

We have removed the phrase "a lower standard deviation" from the sentence.

The standard deviation being referred to is for GIMMS LAI and individual DGVMs, as well as GIMMS LAI and GIMMS NDVI. This has been added to the text. Please see Lines 348 – 353, Page 10.

Line 310: The satellite/NDVI-based LAI is being referred to as "observations". This terminology is used repeatedly in many parts of the paper (e.g. line 321). I think it will be more accurate to refer to this as "satellite-calculated LAI" or something similar rather than as observed LAI, since the satellite-based LAI is also estimated using neural networks on NDVI, and not observed directly.

Response:

We have modified our text to show that "observation-based" or "observed LAI" mean satellite-based LAI, where applicable in the manuscript. We used the word "hereafter" to connect the words.

Line 323: Standard deviation of what? Needs to be specified since there are 2 parameters referred to in the graph.

Response:

We refer to satellite-based LAI and GIMMS NDVI, as well as observation-based LAI and LAI simulated with the DGVMs. The sentence now reads:

Figure 2. Scatterplots of correlations between vegetation indices (observation and model) for the period 1982 – 2011 over southern Africa. Inset values indicate the correlation coefficient (r) and standard deviation (Stdev) between GIMMS LAI and GIMMS NDVI, as well as GIMMS LAI and modelled LAI. The colour represents each grid cell. The pink solid line is the linear regression, while the dashed black line shows 1:1 line. The unequal x-axes is to visualize the detailed data for the models.

Line 352-353: If there is a lag between the LAI and the climatic variables (which is to be expected to some extent as vegetation takes some time to respond to climate drivers), how is this lag in the vegetation response incorporated into the analysis?

Response:

The text in Lines 352-353 discusses Fig. 3 (now Fig. 4), where we compare annual cycle of climate variables and LAI. We consider the lag in our analysis of vegetation response to drought in Figs. 5 – 9 (now Figs 7 – 11). The lag effect is shown as "drought timescale" in these subsequent figures. Please see Line 425, Page 13 (as copied below).

"The lag effect is accounted for in this study, and is known as drought time scale".

Line 361: I am surprised to see an LAI of what seems like approx 0.5 over semi-desert areas. Seems high, is this typical for observed LAI?

Response:

The magnitudes (< 0.5) of the observation-based LAI over (southern Africa) semi-desert biome in Fig. 3 seems "high" because the region is a pseudo-desert, which experiences very high summer temperatures but does receives some rainfall, and the Okavango river is flowing through it permanently. This region is rich in biodiversity such as *Acacia spp* (trees) and *Aristida and Schmidita spp* (savanna) (Please see WWF 2001; Street and Prinsloo, 2013; and referenced in Lawal *et al*., 2018.) Thus, a 0.5 LAI in the biome, which may be higher than other desert biomes, is reasonable in this region. This has been added to text in Lines 713 – 719, Page 27.

Nevertheless, we note that observed LAI is lower than in the rest of the investigated biomes.

Line 369: Why was SPEI simulated? This is unclear

Response:

Simulated SPEI was needed to examine the response of simulated LAI to drought, and thus, we use simulated SPEI to investigate how well the models capture vegetation response to drought. Please see Lines 309 – 312, Page 8 in methodology section.

Line 374-375: It would be helpful for the authors to define what they mean when they refer to the magnitude and intensity of drought.

Response:

Here, magnitude and intensity is on the y-axis of 1-month to 24-month SPEI. Please see Lines 454 – 455, Page 16.

Line 429/Figure 6. It is apparent from Figure 6 that in many cases, the correlation with SPEI of modelled LAI is in average, much higher that that of satellite-based LAI. This is very surprising, that there would be a higher correlation for modelled than observed vegetation indices with SPEI - this seems counter-intuitive that a model would perform "better" than observed data. It would be useful for the authors please discuss this anomaly in the discussion section, it could have some useful implications for the findings. Same with Figure 7.

Response:

The higher magnitudes shown by the multi-model ensemble in Fig. 6 (now Fig. 8) do not mean that the models perform better than the observation-based LAI. It rather shows that models show a larger correlation of LAI and SPEI and might imply that models overestimate the response of the LAI to drought as represented by the SPEI. Same explanation applies to Fig. 7. Aside from overestimating, this could mean that the models are oversimplifying how LAI responds to drought, such that in models, LAI only correlates to water deficit (SPEI).

Line 503/Figure 8. "Spatial correlations of observed LAI response" . It is spatial correlation with what? Please state.

Response:

We have revised it as "Spatial pattern changes of observed LAI (i.e. satellite-calculated) …". Please see Line 615, Page 24.

Line 510: "There is variability in the global and regional temporal distribution of LAI response to drought". Based on the methodology and results presented, the reference to "drought" in this sentence should really read "SPEI" - this should be consistently applied across the paper (including the title), unless significant changes are made to the methodology, as discussed in earlier comments.

Response:

SPEI is an index that is used to quantify drought. Therefore, the quantified values of the index gives the state of drought in a space. Our definition and approaches follow numerous previous studies on the subject, and we have clarified this throughout the text. Please see Line 236 – 240, Page 6.

Line 580-581: The authors spent significant time (lines 540 to 580) discussing the relationship between LAI and NDVI. However, this relationship would be expected given that they derived LAI by applying neural networks to NDVI. It may be better to limit the discussion on NDVI/LAI correlations at a broad level, and highlight and discuss these differences at length for different biomes, and moving that (e,g, Figure S6) from the supplement to the main text.

Response:

Thank you. Fig. S6 in the supplement has been moved to main text. It is now Fig. 5. We have modified and limited the discussion on the differences on NDVI/LAI correlations over different biomes in the manuscript.

Line 582-607: Section 4.2 and 4.3, as presented, do not add to a discussion of the results, and could be removed to reduce space.

Response:

Thank you. Editorial review of the manuscript demanded that we put the text in the discussion. It was necessary to justify the use of LAI and CRUJRA in the study.

Line 609-610: The authors stated that "2014). The frequent and stronger dry spells observed in Fig. 4 could be attributed to climate change." - More analysis (than what the authors presented) is required to support such a statement, it is only a 30 year period that they looked at, and the dry spells do not appear to be getting stronger and more frequent over this period simply from looking at figure 4. For example, the authors could do a frequency analysis, or even just a table showing the number and severity of drought for each 10 year period.

Response:

Thank you. We have added two new tables (Tables 2 and 3) showing the number and severity of drought for each 10 year period to better illustrate how climate change impacts the frequency and intensity of drought over this time period. The severity of drought for each 10 year period is also shown. Please see Lines 469 – 474, Pages 17 – 18. Furthermore, the discussion has been modified to reflect the new findings. Our analyses were adopted after Singh *et al*. (2017).

The tables are copied below:

Table 2. Characteristics of drought occurrence for 1- to 24-month drought timescale for 1st decade (1982-1991), 2nd decade (1992-2001) and 3rd decade (2002- 2011)

Drought Timescale	Number of drought events			Year of moderate drought events		
	1st Decade	2nd Decade	3rd Decade	1st Decade	2nd Decade	3rd Decade
1-month	53	62	53	1987	1992, 1990	2004, 2007, 2008, 2011
3-month	55	64	58	1988	1991, 1992	2004, 2008, 2011
6-month	44	62	60	1982	1992, 1993	2004
9-month	55	66	62	-	1992, 1994	-
12-month	53	68	56	-	1992, 1995	-
15-month	58	63	56	-	1992	-
18-month	54	69	51	-	1992, 1994	-
21-month	51	69	56	-	1992, 1995	-
24-month	41	71	54	-	1992, 1994	-

Table 3. Statistics of the severity of drought for 1- to 24-month drought timescale for 1st decade (1982-1991), 2nd decade (1992-2001) and 3rd decade (2002- 2011). SD is the standard deviation and Max is the highest magnitude of drought occurrence.

Drought Timescale	Mean			SD			Max		
	1st Decade	2rd Decade	3rd Decade	1st Decade	2rd Decade	3rd Decade	1st Decade	2rd Decade	3rd Decade
1-month	0.30	0.39	0.31	0.26	0.26	0.27	1.1	1.02	1.15
3-month	0.32	0.36	0.29	0.24	0.25	0.25	1.02	1.01	1.12
6-month	0.38	0.38	0.26	0.31	0.28	0.24	1.04	1.3	1.33
9-month	0.31	0.41	0.21	0.26	0.35	0.18	0.86	1.32	0.83
12-month	0.30	0.43	0.21	0.21	0.35	0.18	0.81	1.27	0.7
15-month	0.28	0.47	0.20	0.21	0.31	0.18	0.77	1.13	0.7
18-month	0.27	0.48	0.20	0.17	0.28	0.12	1.02	1.02	0.77
21-month	0.18	0.50	0.17	0.2	0.28	0.14	0.62	1.00	0.61
24-month	0.28	0.51	0.15	0.19	0.24	0.11	0.55	0.93	0.46

Lines 627-628: The authors state: "However, SPEI which requires more variables for its computation captures drought better in relatively humid zones (Bengueria et al., 2014)." - It would be useful if the authors could state whether this was their finding in their study too?

Response:

This was shown in previous study – Bengueria *et al*., 2014, so we did not analyze whether SPEI performs better than other drought indices in the present study.

Line 656-658: The sentence needs to be rephrased/corrected.

Response:

Done. This sentence has been repharsed to *"The response of vegetation to drought is particularly stronger in the MAM season, because it is during this period that fruit, leaves and biomass are produced by vegetation (Zeppel et al., 2014)"*.

Line 707: "rounoff" (spelling)

Response:

Corrected.

Line 718: In making this statement, it would be more weighty if the authors could show during the study that the SPEI or its inputs were validated against ground truth, what is its/their accuracy? How accurately does the specific SPEI dataset used measure drought?

Response:

The SPEI was computed from CRU which is an observational-based gridded dataset. Over the different parts of the world, CRU has been widely validated against station data and there is a high accuracy of the validation (e.g. New 1999; 2000; Harris *et al*., 2020). Therefore, observed SPEI gives a high accuracy of measured drought.

Another major advantage of using the observationally-based CRU data is its spatial and temporal coverage. Station data are available for very few points and for limited times in the region of interest. The few data that are available are fraught with missing data, rendering them an unreliable data source.

Archibald *et al*. (2009) & Lawal *et al* (2019a) have evaluated drought parameters such as evapotranspiration and mean temperature from the Skukuza eddy covariance tower site (i.e. measured observation), however, limited spatial and temporal coverage makes it difficult to identify and understand drought characteristics. The Skukuza FLUXNET site in southern Africa is available at small spatial coverage (Latitude: -25.0197, Longitude: 31.4969) over an 11-year period (i.e. 2000–2010).

In addition, the measured data are often different from what are needed to compute drought index. For instance, the FLUXNET data from the Skukuza tower site measures actual evapotranspiration (ET_o) and not potential evapotranspiration (PET) which is used to calculate SPEI. Please see below Fig. S8 for time series comparison of precipitation and mean temperature from Skukuza, CRU and CRUJRA (as copied below). The discussion has been modified to reflect this addition. The figures show that CRU and CRUJRA characterize the dry season with less precipitation. This suggests that the droughts might be stronger in CRU than station or tower data would suggest. The following text has been added to the revised manuscript.

"Comparing CRU to flux tower precipitation in Skukuza (Kwazulu Natal region) illustrates that CRU captures the timing of precipitation relatively well, though underestimates the magnitude of dry season precipitation (Fig. S8)". Please see Lines 861 to 865, Page 31.

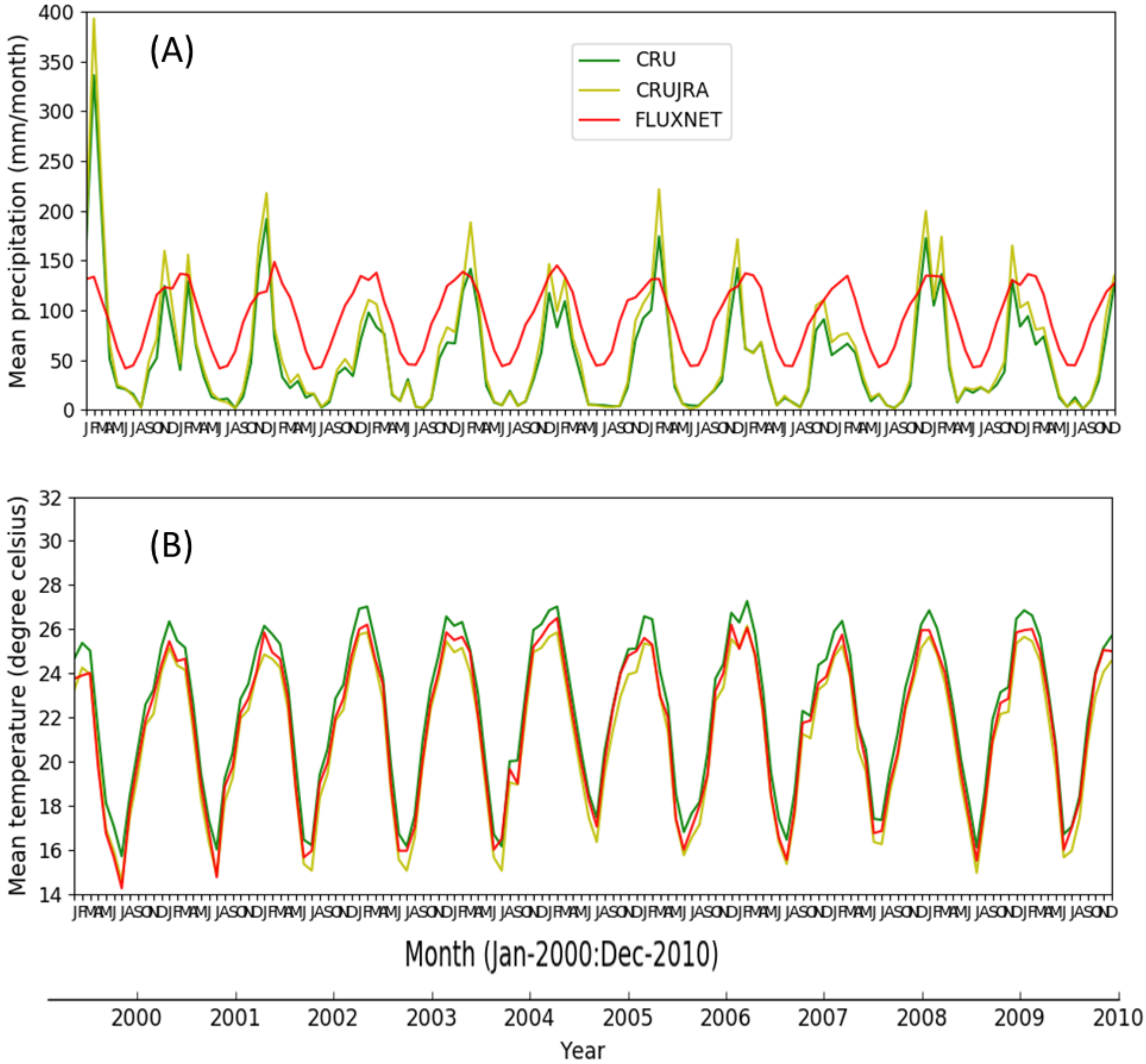

Fig S8. Monthly distribution of (a) precipitation (mm/month) and (b) mean temperature (°C) for eddy covariance (measured observation) over the Skukuza tower site, CRU and CRUJRA (latitude:-25.02, longitude: 31.50). The variables were plotted for the period 2000 – 2010.

Line 721: "mostly at a shorter time period (3-, 6-month timescale)": Does this statement refer to the second part or both parts of this sentence?

Response:

Both. Revised

Line 722: "at 6-month timescale in the MAM season". The idea of a 6-month timescale in a 3-month (MAM) season is a little difficult to conceptualize, perhaps the authors can rephrase.

Response:

Thank you. This has been clarified in the manuscript.

Line 732: "The relationship between the NDVI and LAI is linear thus implying..." Given that the LAI was derived from NDVI, this statement seems a little bit redundant, or perhaps I am misunderstanding something.

Response:

Despite the linear relationship between the NDVI and LAI, we felt it was necessary to discuss the correlations of these indices, considering the fact that GIMMS LAI product is superior here over the GIMMS NDVI, which is due to the information derived from the MODIS LAI. The additional properties on GIMMS LAI by MODIS differentiate the index from NDVI.

Line 739-740: this statement is difficult to understand. Please rephrase.

Response:

Revised.

Line 742: "There is a stronger LAI response to drought in dry years than in wet years...". please rephrase. the statement is difficult to comprehend given that drought occurs in dry years not wet years.

Response:

Thank you. This has been revised.

Line 756-757: "While this study may have provided an insight into the capability of DGVMs to simulate vegetation response to drought". This sentence seems incomplete.

Response:

The sentence has been combined with the next sentence to make it complete.

Line 760-761: I dont think using more models will reduce uncertainty, but perhaps may allow it to better quantified.

Response:

The suggestion has been added to the text.

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